produce evidence confirming this last statement. I admit that carefully studied and conclusive instances are not very numerous, but I refer to such cases as the non-transmission (*a*) of plus or minus variation in pigment produced in individuals by greater or less exposure to sun-light; (*b*) the effects of dry or moist climate on individual plants; (*c*) the effects of change of diet on individual animals; (*d*) the effects of increased use of muscles in men and animals.

It seems that we are driven to the conclusion that the causes which have been active in producing changes the accumulation of which amounts to specific, generic and larger differences, must be causes which are able to act upon the potential congenital quality of the individual, and that there is no reason for associating the somewhat superficial and late responses or reactions of the parts of a growing individual to normal or abnormal forces of its environment with that more subtle and profound disturbance, which is permanent and affects the potential character of the germ, and more or less of all the germs derived from it.

At any rate this is the absolutely unanimous testimony of all those observers, in all countries and in all ages, who have been practically concerned (often with vast pecuniary interests at stake) in the production of relatively permanent new races of animals and plants. Breeders of horses, cattle, sheep and dogs, pigeon and poultry-fanciers, crop-growers, nurserymen, tulipomaniacs, and the like, have never in any single instance put the Lamarckian principle into practice. On the contrary, they laugh it to scorn. Not one of them ever produced a new race by moulding the parents. But, on the other hand, they *do* subject the selected parents to novel and disturbing conditions, to which the changed characters of the *offspring* (not of the parents) have no "responsive" relation; they cross-breed here and cross-breed there, until the "specific potential" is broken-down, and strange unlooked for varieties are born and grow up irrespective of normal or abnormal environment. From these congenital variations they select desired forms, and perpetuate them with perfect assurance and security.

For the present I see no evidence of a production of new races on the face of the earth, excepting by the method adopted by these men, viz. by the selection of congenital variations; such congenital variations being produced as the result of (but without any direct adaptational relation to) a disturbance of the material of the reproductive particles of both sexes; that disturbance being increased, if not determined, by changed environment of the parental organisms or the coupling of remote strains.

E. RAY LANKESTER.

Oxford, November 17.

## The Present State of Physiological Research.

THE extracts given in NATURE of November 15, from an article by Prof. Max Verworn, of Jena, on "Modern Physiology," will serve to draw the attention of biologists to the reawakening of interest which is now evinced by many physiologists in regard to the fundamental phenomena exhibited by living things. As the opinion of physiologists is expressly invited in reference to the questions raised in this article, I venture to express my own as being in the main the same as that of Prof. Verworn.

It seems to me an obvious truism to say that the methods which can ensure a real advance in general biological knowledge must be those in which comparative physiology takes the lead. In my recent presidental address to the Liverpool Biological Society I urged the establishment of laboratories for the systematic study of the comparative physiology of the simpler organisms, the end in view not being the elucidation of the functions of organs with an *arrière pensée* as to their relation to man, but the examination of the activities for their own sake, since this inquiry forms the only means of approaching the mystery which enshrouds the essence of living existence.

The determination of the reactions of simple organisms to physical changes (stimuli), and the grouping of such resultant effects carried out systematically, form a line of physiological inquiry of transcendent importance, both because of its large scope and fundamental character, and because it opens the way towards the partial elucidation of the physiologist's real problem. This problem is the one involved in the question, to what extent all living phenomena are to be regarded as reaction phenomena? Are we, on the other hand, compelled to postulate the existence in every living thing of a *deus ex machina* which can, if it will, act independently of every physical stimulus, a so-called "vital force"?

Prof. Verworn is right in forcing upon the attention of physiologists the paramount necessity for work of this kind. I venture, however, to point out that he has not done justice to the judgment of his contemporary physiologists if, as I imagine, he has been led to infer from the character of the mass of current physiological work, that they do not realise the importance of such comparative physiology. But to realise the importance of an inquiry and to be able to carry that inquiry into effect, are unfortunately by no means identical positions.

There are undoubted obstacles to the latter, however ardently we may desire its fulfilment.

In the first place physiology is, in this country, more or less shackled by its position. It owes everything to medicine. Its laboratories are adjuncts to medical schools, its professors must take their share in the teaching in such schools, and this teaching is essentially connected with human physiology.

The debt, which as physiologists we owe to medicine, is one which we gratefully acknowledge, but even with the thanks on our lips we may be supremely conscious of the chains which still hang on and impede the debtor. It is this close relationship which, in my opinion, has served to accentuate the separation between physiology and the science of which it properly forms part, biology; a separation which is now almost a judicial one, and if unchecked may become an actual divorce. It is rare to fine a physiologist who has been highly trained in zoological investigation, and rarer still to find a zoologist who has attempted to perfect himself in the methods used in physiological laboratories. Yet the appropriate blend is essential for the advent of those comparative physiologists who alone can do full justice to the systematic inquiry now advocated.

Another difficulty in this country is undoubtedly due to the scanty pecuniary help afforded to scientific work which is neither technical nor directly concerned with what is regarded as the public good. Physiology, to-day, is maintained in Great Britain solely because it forms an essential part of a specialised technical education, that of the medical student; it is not maintained in order to inquire into the mystery of living things as such.

In order to adequately develop such an inquiry as this, it would be necessary to have a new department furnished with the equipment of both a zoological and a physiological laboratory, and with skilled workers who have leisure to prosecute their investigations. Since this means money, its full establishment may have to be postponed until that pious benefactor appears whose dawn even a Bodleian librarian has now anxiously to await.

Finally, I do not think the outlook is so discouraging as Prof. Verworn seems to believe, nor that "we are making no progress in physiology."

He admits that during the last twenty years we have attained to a precision in our experimental methods such as excites the astonishment of the uninitiated; and surely the mastery of method is the first step, and that an invaluable one, towards its future more fruitful employment. I do not imagine that even the systematic physiological investigation which he advocates, will involve the employment of new methods to the exclusion of old ones; it is the material which will be novel, not the entire experimental technique. Isolated instances of the application to simple excitable organisms, of such physiological methods as have been employed in elaborate detail for the investigation of muscle, nerve, &c., are well known to us all, and to no one better than Prof. Verworn; the real desideratum is surely that the instances should be no longer isolated, but form part of a broad systematic inquiry.

FRANCIS GOTCH.

University College, Liverpool,
November 17.

## Wilde's Theory of the Secular Variation of Terrestrial Magnetism.

MR. WILDE'S reply in NATURE of October 11 to my letter of criticism in the same of August 9, with respect to his communication to the Royal Society, contained in the *Proceedings* for March 1894, has just come to my attention.

As the letter consists entirely in an attempt to show the inaccuracy and unreliability of my statements with respect to the inclination-observations made at St. Helena, behoves me to confine my own remarks to this matter. In order that my explanations may be understood, the table previously given is here reproduced.

| No. | Date. | Observer. | Observed inclination. | Wilde's inclination. | Observation – Theory. |
|---|---|---|---|---|---|
| 1 | 1700 | Hansteen Chart | 2° S. | 3°·9 N. | –5°·9 |
| 2 | 1754·3 | La Caille | 9·00 | 0·5 S. | –8·5 |
| 3 | 1771·4 | Ekeberg | 13·00 | 3·5 | –9·5 |
| 4 | 1775·4 | Cook | 11·42 | 4·0 | –7·4 |
| 5 | 1780 | Hansteen Chart | 10·5 | 5·1 | –5·4 |
| 6 | 1825·0 | Duperrey | 14·93 | 14·7 | –0·2 |
| 7 | 1840·1 | Ross | 18·27 | 18·5 | +0·2 |
| 8 | 1842·3 | Belcher | 17·00 | 19·0 | +2·0 |
| 9 | 1846·8 | Smyth | 19·39 | 20·5 | +1·1 |
| 10 | 1890·1 | U.S.C. & G.S | 29·65 | 33·8 | +4·1 |
| 11 | 1890·1 | ,, | 31·18 S. | 33·8 | +2·6 |

This table is in every respect the same as originally given with the exception of the value for 1700, the erroneous value of 11°·5 S. having been given instead of 2° S. Owing to my sojourn in Europe being but a temporary one, I have not with me all my data, and so cannot ascertain definitely how this error crept into my table. My original scaling was probably 1·5 S., which by a copying blunder may have been converted into 11·5. This, however, I cannot control now. It would have been a most natural inference on Mr. Wilde's part if he had ascribed this to the "printer's devil," all the more so as no use whatever was made in the text of my communication with this value. On account of so apparent an error he casts a slur upon my trustworthiness in general in regard to terrestrial magnetic matters. Such a poor method of argument reveals the weakness of his position.

Furthermore, with reference to this table, Mr. Wilde says: "I regret to observe that L. A. Bauer, in his intolerance of the magnetarium results, has inserted in his table guesses of his own for observations, which are very wide of the truth." I can find no excuse whatever for this statement. Mr. Wilde has acknowledged that he possesses a copy of Hansteen's "Magnetisms der Erde." Let him turn to Tafel II.: "Neigung der Magnetnadel," p. 36, and he will find the following observations given for St. Helena:—

| Observer. | Date. | Inclination. |
|---|---|---|
| De la Caille . . | April 10, 1754 | 9° 0′ S. |
| Ekeberg. . . . | May 19, 1771 | 13 0 S. |
| Cook . . . . | May 17, 1775 | 11 25 S. |

Position assigned by Hansteen: latitude 15° 55′ S., longitude 11° 52′ E. of Ferro, or 354° 12′ E. of Greenwich. By inspecting the table given above it will be seen that these form Observations Nos. 2, 3, and 4. No. 5, as stated, is taken from Hansteen's Chart for 1780. Mr. Wilde does not appear to question this value, nor the remaining ones, which can be easily found in Sabine's "Contributions to Terrestrial Magnetism." He will, furthermore, find that Hansteen had so much faith in the early observations, which Mr. Wilde insinuates are untrustworthy, that in 1857 he made use of all the observations known up to that time, viz. Nos. 2, 3, 4, 6, 7, 8, and 9, for the establishment of a periodic formula representing the secular variation of the inclination during this epoch. These investigations of Hansteen's can be found in "Den magnetiske Inclinations Forandringer i den nordlige og sydlige Halvkugle af Christopher Hansteen," Copenhagen, 1857, –4°. Hansteen, by a least square adjustment of the observations named, derived the following interpolation formula:—

$$i = -13°58'·455 - 5'·44405\,(t - 1800) - 0'·001013\,(t - 1800)^2.$$

$i$ denotes the inclination at the time $t$, south inclination being reckoned as minus. This formula at the utmost should not be used more than ten years prior to 1754, nor ten years later than 1846. The following table shows how the values computed with this formula agree with observation.

| Date. | Observed inclination. | Computed inclination. | O. – C. |
|---|---|---|---|
| 1754·28 | – 9°·00 | – 9°·85 | +0°·85 |
| 1771·38 | –13·00 | –11·38 | –1·62 |
| 1775·38 | –11·42 | –11·77 | +0·35 |
| 1824·96 | –14·93 | –16·26 | +1·33 |
| 1840·10 | –18·27 | –17·66 | –0·61 |
| 1842·35 | –17·00 | –17·87 | +0·87 |
| 1846·79 | –19·39 | –18·28 | –1·11 |

From this comparison it will be seen that the formula represents the observations fairly well. Let us compute then with it what the inclination would be in 1747. We obtain –9°·2. Now Mr. Wilde's magnetarium has given us for this date the value 0°·0. Hence there is an outstanding difference of about 9°, which he has made no attempt to explain otherwise than by insinuating that I have put "guesses" in my table, or that the observations are untrustworthy. The burden of the proof that the observations are not trustworthy, rests with Mr. Wilde. Anyone, who has made any endeavour to familiarise himself with the literature of the subject of terrestrial magnetism, will know that it is an old story for theorists to characterise observations as doubtful if they do not happen to agree with their theory. I am willing to admit that the early inclinations in such a locally disturbed region as St. Helena, perhaps, cannot be depended upon nearer than to 2° or 3°; but, if Mr. Wilde will pardon my scepticism, I do not believe that it is possible for him to reproduce any inclination with his magnetarium that can be relied upon even to this extent. Hence, it is fair for me to compare the magnetarium results with that of observations (so long as the latter have not been overthrown) without consideration of the probable error of either result.

Mr. Wilde appears to have thought his position proven when he found that I made an error with respect to my first value. But even with the value as given by him, the outstanding difference is 5°–6°, with which he appears perfectly satisfied. If he will permit me a probable error as large as he permits himself in the establishment of his theory with his magnetarium, I can supply him with a dozen periods that will satisfy observations as well as his magnetarium. I would like to refer him to a recent attempt by Dr. Felgentraeger, who endeavours to prove the universality of the secular period by establishing periodic formulæ upon the basis of most carefully collected material. He deduces a period of 477 years—instead of Mr. Wilde's 960—upon the basis of the declination observations made at London 1580–1882, and Paris 1541–1890.[1] Adopting this period he found that he could represent exceedingly well the observations made at London, Paris, Rome, Clausthal, Chambersburg (U.S.A.), Rio de Janeiro, Cape of Good Hope, and Cape Comorin. Here we have a more extensive comparison than Mr. Wilde has given us, and we find a better agreement with observations with a period one-half of his! In this brief communication I cannot set forth my own position with respect to the secular-variation period. I hope to present Mr. Wilde with a copy of my investigations some time in December.

With respect to my opinion of the magnetarium in particular, I may say that my criticisms made thus far have applied solely to the theory as evolved from the magnetarium results, and do not touch the magnetarium as a valuable instrument of research. Indeed, I think much good can be accomplished with it. Mr. Wilde has made a most laudable attempt to reproduce mechanically the complex phenomena of terrestrial magnetism, and if the achievements with his ingenious mechanism had not received the publicity they did, or had been properly interpreted, my criticisms would never have been made. That he has not succeeded in giving us a better representation is no fault of his, but owing to the complexity of the phenomena.

The fact that Mr. Wilde has succeeded, by an arbitrary distribution of magnetic matter in his magnetarium, in representing the *distribution* of terrestrial magnetism for the year 1880 apparently so well, is no proof of his secular-variation theory or his period, which plays no part in determining the distribution. Nor is the fact that with *his* distribution of magnetic matter he gets a good representation a proof that *that* is the actual distribution prevailing in the earth. It can be demonstrated as a mathematical fact that in the absence of terrestrial magnetic observations within and without the earth's surface, an infinite number of different distributions is possible that will satisfy the effects observed on the surface alone.

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[1] Dr. W. Felgentraeger: Die längste nachweissbare Säculare Periode der erdmagnetischen Elemente, Teil 1: Deklination. Inaug. Diss. Universität zu Göttingen, Göttingen, 1892. Buchdruckerei von Louis Hofer.

In conclusion, it is my duty to make one more explanation. Mr. Wilde understood from my first letter that I am still in the employ of the U.S. Coast and Geodetic Survey. In view of the fact stated by him, that he sent, at considerable trouble and expense to himself, a duplicate of his magnetarium to the "Survey," and, hence, it might appear that it was somewhat discourteous in a member of the "Survey" to thus criticise him publicly, I may say that I severed my connection with the Survey two years ago, and that my criticisms have been made without any knowledge whatsoever of what has been accomplished with it by the "Survey."

Friedenau bei Berlin, October 31. L. A. BAUER.

### Boltzmann's Minimum Theorem.

MR. CULVERWELL's letter of October 25 ought to have received a much earlier answer. That it did not do so was owing to purely accidental circumstances which I very much regret.

In that letter Mr. Culverwell criticises my treatment of Boltzmann's familiar proposition concerning the properties of the H function on the following grounds:—

(1) The choice of the generalised coordinates. In investigating the circumstances of a collision or an encounter between two systems of molecules of $m$ and $n$ degrees of freedom respectively, he sees a difficulty in my choice of the coordinates as $Q_1, Q_2 \ . \ . \ . \ Q_m, q_1, q_2 \ . \ . \ q_n$, where $(q_n = a)$ determines a collision, or encounter. But supposing the requisite number of degrees of freedom to be secured, the choice of the independent variables is surely quite optional. I had myself assumed this as self-evident, perhaps too hastily, but at any rate Mr. G. H. Bryan has placed this proposition beyond doubt, in the exhaustive report submitted by him to the British Association last August. Take for example sets of plane circular disks moving amongst each other in their own plane; here each pair of disks constitutes a material system, whose position is completely determined (assuming the orientation of each separate disk to be indifferent) by the following four variables, viz. the two coordinates of the centre of one disk of the pair, the distance $\rho$ between their centres, and the inclination of that distance to a line fixed in the plane; this third variable $\rho$ is the $q_n$ of my proposition.

(2) Mr. Culverwell objects that the general Boltzmann proposition $\left(\frac{dH}{dt}, \text{ always negative unless } Ff = F'f\right)$, or H a minimum for one, and one distribution only, cannot be true, because if a system were started from any initial configuration (P, Q), and after the time $t$ arrived at the configuration $(p, q)$ and the definite integral H were evaluated in these two configurations, the proposition asserts that the second H must be less than the first H, or $H_t < H_o$, whence it would follow by the same proposition that if at the end of the time $t$ each velocity component were reversed, the $H_{2t}$ must be less than $H_t$, and this, doubtless, I do assert. But Mr. Culverwell maintains that such an assertion is obviously untrue because at the end of the second interval $t$ the system has returned to its *original* condition, and therefore $H_{2t}$ must be the same as $H_o$, and to this proposition I demur.

Doubtless when a material system in a field of any conservative forces is started from any initial position and velocities, arrives after a time $t$ at another position and with other velocities, and here has each velocity reversed, it is true that at the end of the next interval $t$ it will be in its initial position, with each velocity component reversed; but it remains to be proved, and cannot be asserted *à priori*, that the $H_{2t}$ is equal to $H_o$, and the only proposition available for the investigation is this very proposition of Boltzmann's, which proves that $H_{2t}$ will be less than $H_t$, and therefore less than $H_o$.

Finally, Mr. Culverwell inquires, somewhat despondently, if anyone will point out the use of the H function, and what is proved by it.

I have already said in my second edition, that the proposition is not of my invention, and therefore that I have no claim to answer this question with any authority, but to my own mind the proposition appears certainly to clear away one obvious difficulty. Without the aid of this proposition we are enabled to assert that if F $(p, q)$ were a function of the coordinates and momenta of a molecule such that in the absence of encounters *with other molecules*, F remains constant for all time, then the form of F satisfying the condition $Ff = F'f'$ must render F $(p, q)$ $dp\ dq$ a *permanent* law of distribution, and therefore if we can assert that F $(p, q)$ must *of necessity* be of the form F (E) (E sum of potential and kinetic energy), then the $e^{-hE}$ law of distribution is certainly a permanent law (neglecting, *i.e.*, all but binary encounters); but in the absence of this proposition, we cannot assert that the $Ff = F'f'$ is *necessary* as well as sufficient, because we cannot insist upon the necessity of an exact compensation in the passage from the $p\ q$ to the $p'\ q'$ state, and conversely, taking place at *each separate* encounter. The H proposition, therefore, removes this element of uncertainty, and reduces the question to that of the F (E) restriction, because it proves that unless $Ff = F'f'$ for *each pair* of encountering molecules, H and therefore F and $f$ must be a function of the time. As I have already asked for a disproportionate share of your space, I will not enter upon the question of the F (E) restriction now.

H. W. WATSON.

I DID not exactly, as Mr. Burbury suggests, question Boltzmann's minimum theorem, but only the pages thereon in Dr. Watson's "Kinetic Theory of Gases." Indeed, I said that though I had not seen Boltzmann's proof, I supposed it to be all right.

It appears from Mr. Burbury's letter that in order to prove the theorem, even for the simple case of perfectly hard and elastic spheres, some amount of assumption as to an average state having been already attained must first be made, and of course the *à priori* reasoning in my letter is not applicable to such a theorem. Mr. Burbury's letter is exactly the kind of letter I hoped to elicit, and if he can say what assumption in a generalised system will replace the assumption of equal distribution of velocities in different direction in a system of hard spheres, he will clear up the whole difficulty. Unfortunately, the case with which he deals is one in which the error-law is known from other considerations to be the only permanent state.

I observe that Mr. Bryan, in his British Association Report, quotes the oversight I pointed out in Dr. Watson's proof, without making any criticism on it.

EDWD. P. CULVERWELL.

Trinity College, Dublin, November 24.

### The Alleged Absoluteness of Motions of Rotation.

PROF. GREENHILL, in his review of Mach's "Science of Mechanics" (NATURE, November 15), writes as if he disapproved of that author's not accepting "Newton's distinction between the relativity of motion of translation and the absoluteness of motion of rotation." He appears to think that Mach would have obtained more insight into this distinction from a study of Maxwell's "Matter and Motion." It might more truly be said that Maxwell would have profited by a perusal of Mach's book The latter finally refutes the paradox contained in Newton's statement, and supported by Maxwell, and by so doing renders a great service to Mechanical Science. He has disposed once and for all of "absolute rotation." It is high time that writers on Mechanics should revise the preliminaries of their science so as to state their results in terms of relative motion, whether of translation or rotation. This has been partially done by Maxwell, and a further step has now been taken by Mach. It is unfortunate that the reviewer in drawing attention to this part of the book should have preferred to stand by the prejudice he owes to Newton and Maxwell when he might have done something to hasten its abandonment.

A. E. H. LOVE.

St. John's College, Cambridge, November 20.

MACH says truly (p. 237) "that precisely the apparently simplest mechanical principles are of a very complicated character; that these principles are founded on uncompleted experiences, which can never be fully completed," &c.

The modern student of theoretical mechanics is in a dilemma;